



## Early Journal Content on JSTOR, Free to Anyone in the World

This article is one of nearly 500,000 scholarly works digitized and made freely available to everyone in the world by JSTOR.

Known as the Early Journal Content, this set of works include research articles, news, letters, and other writings published in more than 200 of the oldest leading academic journals. The works date from the mid-seventeenth to the early twentieth centuries.

We encourage people to read and share the Early Journal Content openly and to tell others that this resource exists. People may post this content online or redistribute in any way for non-commercial purposes.

Read more about Early Journal Content at <http://about.jstor.org/participate-jstor/individuals/early-journal-content>.

JSTOR is a digital library of academic journals, books, and primary source objects. JSTOR helps people discover, use, and build upon a wide range of content through a powerful research and teaching platform, and preserves this content for future generations. JSTOR is part of ITHAKA, a not-for-profit organization that also includes Ithaka S+R and Portico. For more information about JSTOR, please contact support@jstor.org.

versy. Then, perhaps, Dr. Merriam will tell us whether he continues to recognize *Lepus americanus* and its subspecies *L. a. virginianus*.

SAMUEL N. RHOADS.

ACADEMY OF NATURAL SCIENCES,

PHILADELPHIA, April 17, 1896.

AMERICAN POLAR HARES: A REPLY TO  
MR. RHOADS.

THE above wail from Mr. Rhoads respecting my review of his paper on the Polar Hares calls for a brief reply. It was not the importance of Mr. Rhoads' paper, as he seems to suppose, but the importance of certain principles involved in his methods of treatment, that led to the length of my review. My criticisms were aimed mainly at two matters: one, a matter of description; the other a matter of nomenclature. In describing the new American hares, Mr. Rhoads contrasted them with a European species (*Lepus timidus*) instead of with their American relative (*Lepus glacialis*). This struck me as bad systematic zoölogy. In treating the Polar hare of Baffinland he adopted the specific name *arcticus* instead of *glacialis*, though both names appeared simultaneously in the same book. This struck me as bad nomenclature.

The reasons for retaining *glacialis* as the proper name of the animal were stated at length in my review and need not be repeated here. But in his reply Mr. Rhoads implies that I have subordinated priority to the scientific standing of an author. This I deny. Priority of publication is the cardinal principle of nomenclature—the foundation of all modern codes; without it, stability in nomenclature is impossible. But priority of *publication* and priority of *pagination* are two widely different things, and I deny that priority of pagination constitutes priority of publication. It can hardly be gainsaid that the different pages of a book appear simultaneously; hence names on different pages of the same book should be treated in the same way as names appearing simultaneously in different books. Sequence of pagination is a trivial circumstance, not to be considered in fixing specific names except in cases where no other reason for a choice can be found. Even the A. O. U. Code quoted by Mr. Rhoads concedes this, and goes so far as to

accord greater weight to *sex*, *age* and *season* of the type specimen than to priority of pagination. In other words, in choosing between names of even date, sequence of pagination is a last resort.

It is useless to enter into a controversy with Mr. Rhoads over his astonishing statement that of the descriptions of the American Polar hare given by Ross and Leach, "Ross' description is the better of the two." Reference to the work in which both appeared will settle this point.

In reply to Mr. Rhoads' inquiry as to the source of the rule that 'in cases of equal pertinency the first reviser of the group has the privilege of fixing the name,' it may be stated that said rule expresses the practice of most systematic zoölogists—and I think botanists as well—and is in complete accord with the spirit of the A. O. U. Code, though not there formulated as a distinct canon. In closing, I must thank Mr. Rhoads for calling my attention to what he considers would have been a proper review of his paper.

C. H. M.

THE SUBJECT OF CONSCIOUSNESS.

TO THE EDITOR OF SCIENCE: In the number of SCIENCE for May 15th there is a letter from Johannes Rehmke on the subject of 'consciousness,' about which I beg leave to be indulged in a brief statement.

Take two equal weights with handles, one weight being several times the bulk of the other. Ask a blindfolded man to tell which is the heavier, being careful not to let him touch either weight, but only the handle, and he will not judge of a difference. Now let the same man, seeing the weights, but not knowing them to be the same, decide which is the heavier; he will affirm that the smaller is the heavier weight. This is a common experiment in psycho-physics. There are on record a vast number of similar experiments which have been abundantly verified, all leading to the conclusion that there are two elements in sensation, the one of consciousness of the effect upon self and the other an inference relating to the thing observed by any one of the senses. All of these experiments, and a vast body of experiences which every individual undergoes,

testify to these two elements. At the last meeting of the National Academy I presented a paper on this subject, from which I extract the opening paragraphs, as follows :

All operations of the mind are judgments. On examining the nature of the judgments we discover two elements or functions, consciousness and inference. Consciousness is awareness of self and change in self, and inference is a guess at the cause of the change. We can discover these functions or elements in all of the judgments of mind. I am conscious of a sound; I infer that it is the voice of a friend. I am conscious of an odor, and infer that it is caused by a rose. I am conscious of a flavor, and I infer that it is the taste of an apple. I am conscious of a sense impression of color, and I infer that it is caused by a tree. These judgments may be erroneous and I may believe in illusions, but in every case a judgment is formed, whether correct or incorrect. The condition under which judgments produce illusions or certitudes will hereafter be set forth. That which we have to consider now is that in every mentation, whether true or false, as in the perceptions mentioned, there is a consciousness and an inference. It will be noticed that we have defined the term consciousness as awareness of change in self, and to this definition we shall adhere. The word is used in many other senses, but in science it becomes necessary to use words with a single meaning. For example, we might use the term consciousness to mean also the cognition of self or another, and it is often used in this manner as a general synonym for cognition, but we must have some term to designate awareness of the change in self and select the word consciousness for that purpose, as that seems to be its fundamental meaning.

A consciousness is awareness of change in self, so inference is the interpretation of the meaning of that change. A change has been effected upon my organ of hearing, and I am conscious of a sound and interpret it as a voice; this interpretation is inference. It is not a random guess, but a guess dictated by experience or some collateral circumstance which suggests this guess. Consciousness, therefore, is not only independent, but it is also absolute in the sense that it must have reality as a change in self; the inference is not only dependent, but it is also subject to error. It may be a certitude or it may be an illusion. Thus, there is either a certitude or an illusion produced by an inference. How then does the mind distinguish between certitudes and illusions? Here we have to consider cognition.

Verification is the proof of the inference by experience. Cognition is composed of three functions: consciousness, inference, and verification. That

which is produced by cognition is certitude. A judgment is composed of two functions—consciousness and inference; if verification is added by experience it becomes a certitude; if it is not verified by experience it is proved to be an illusion. These may seem very simple propositions and self evident, as they are, yet they are fundamental and must be clearly understood in order that proper progress may be made in the study of cognition.

What I have designated as consciousness and so defined the term Rehmke designates as subject of consciousness; what I have defined as inference he calls attribute of consciousness. But I go on to use judgment in a restricted sense as based on a consciousness and an inference, and then use cognition as a mentation of three elements—consciousness, inference and verification. As I understand Rehmke's method of defining the two terms of consciousness, he makes a valid distinction which is fundamental in psychology and if properly and rigidly observed dispells many illusions in psychology, and experimental psychology has abundantly demonstrated Rehmke's position.

I regret that I have not seen Rehmke's book, and on consulting the four papers of SCIENCE for last September I do not discover that it was reviewed therein as indicated by his remarks.

In the judgments formed in the experiment with the two weights the blindfolded man makes a judgment of relative weights; the seeing man makes a judgment of relative specific weights. Having in advance seen the weights, he has already formed a judgment and uses this judgment of sight in interpreting the consciousness experienced through the sense of muscular strain. The psychology of sensation and perception cannot be understood or explained without using distinct, definite and understood terms for what I have called consciousness, inference, judgment, verification and cognition. What terms shall be used matters little; it may be that Prof. Rehmke's use of subject of consciousness and attribute of consciousness is wise, but I fear that it will make still greater confusion in a subject which is already burdened with terms, and it seems to me better to follow the example of the physicists in giving restricted meanings to words already in use, as in the case of momentum, energy, force and

power, and then rely upon the acceptance of the terms with the restricted meanings.

J. W. POWELL.

WASHINGTON, D. C., May 16, 1896.

#### SCIENTIFIC LITERATURE.

*Text-book of Comparative Anatomy.* By ARNOLD LANG. Translated by H. M. and M. BERNARD. Part II. London and New York, Macmillan & Co. 1896. 8°. Pp. xvi+618, with many illustrations.

The second part of this well-known text-book has been impatiently awaited by teachers of invertebrate anatomy and those who desired a convenient work of reference summarizing the essential facts of the science. Among the numerous text-books of this sort which have appeared of late years, each of which has had its especial merits, that of Lang has reached an easy preëminence, on account of the wide erudition and judicial temper with which the different topics are treated. It is, of necessity, in one sense, a compilation and the chief criticism which has been made upon the German edition is that the authorities for the facts used are cited in mass as literature and not in connection with the particular data due to each. Prof. Lang explains that considerations of space made this obligatory, though, naturally, the work, as a book of reference, would have gained in value as well as size by specific citations. The translation, on the whole, is easy and idiomatic, only occasional Teutonicisms are noted, though it would seem as if some more apposite term than 'Appendage' might have been used for the supplementary chapters on *Rhodope* and *Rhabdopleura*. The typography of the English edition is much more tasteful than that of the original; the illustrations are well printed, and the work will doubtless receive a wide and merited acceptance as a text-book. The present volume includes *Mollusca*, *Echinodermata* and *Enteropneusta*, but the special criticism on this occasion will be confined to the mollusks.

It would be superfluous, perhaps, to criticise in this place the general plan upon which such text-books are constructed, but it cannot be denied that the comparison, organ by organ of a multitude of animals, leaves a somewhat incoherent impression upon the mind. As things

are constituted, anatomists are rarely systematists and the systematic part of any of the manuals leaves much to be desired by the specialist. The ideal comparative anatomy would relegate the specific facts to eminent specialists and the comparisons to a systematic genius as editor, a state of beatitude which we are far from approaching.

Prof. Lang is not an eminent specialist in mollusks, but he has a wide knowledge of the literature, and his remarks on mooted points are generally characterized by good sense and sound judgment. The compendium may be said to be, as a whole, representative of the date of 1889, though, in some instances, the text shows later references.

In selecting an architypal mollusk with which to compare his actual animals, the author has followed Lankester's hypothesis of 1884. The archetype is regarded as an animal somewhat between *Fissurella* and *Chiton*, bilaterally symmetrical with a posterior vent and straight alimentary canal. We are of opinion that Prof. Verrill's suggestion that the architypal mollusk in the main conformed to the type of the molluscan veliger, with a bent intestine and anterior vent, is much more in harmony with our knowledge of the facts; but space forbids a discussion of the question here. The classification of the Pelecypods is adopted from Pelsseneer, whose method has been of late pretty thoroughly tested and found wanting, though at the time this text-book was in the making, it was the newest and presumably the most satisfactory. On the whole, however, Prof. Lang has succeeded in bringing together the data in an excellent manner, and the cordial reception of the German edition is sufficient evidence of the estimation in which his work is held by his scientific colleagues.

Since this work will undoubtedly take a prominent place among the text-books used by teachers, it will not be regarded as hypercriticism to use the remainder of our space in pointing out such items as, on a general perusal, have appeared contestable, erroneous or obsolete. Any work of this kind necessarily contains a certain percentage of such slips, and their presence cannot justly be regarded as condemning it above its fellows. Their correction, therefore,